AN

ESSAY

ON THE

CIRCULATION OF THE BLOOD.

AN ESSAY

ON

THE FORCES

WHICH CIRCULATE THE BLOOD;

BEING AN EXAMINATION OF

THE DIFFERENCE OF THE MOTIONS OF FLUIDS IN LIVING AND DEAD VESSELS.

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quirements and your talents, you have been an honour to our profession. But, above all, I feel grateful to you, that you have distinguished that part of the profession to which you belong, from a class of writers who, in seeking to associate themselves with distinguished men in philosophy, have adopted the worst part of what is peculiar to them. In France, during the period of the Revolution, two of the most celebrated anatomists and physiologists of that country were carried with the prevailing fashion of the time; and incorporated into their works the sentiments of Helvetius, Mirabeau, and other philosophical writers. These sentiments may be traced, like a turbid stream in pure waters, through some of the works of our own country. Originally these were not the opinions of the students of nature, but of secluded men; and the morbid excess of feeling which drove them to retirement, gave to the expression of their concep-

tions a fervour which might be their apology. But when, without that embellishment which genius can throw over them, we find them unnaturally joined to the demonstrative evidences of anatomy, they appear to me, as they have done to you, false in themselves, and quite foreign to the occasion.

No man ever received more hearty applause than you did in the conclusion of your discourses to the College; because you spoke what every man felt to be justly conceived, as well as eloquently delivered. You adapted yourself to the more matured judgment of the College; and you carried the hearts of the students with you by the spirit and animation of your discourse. Had you done violence to our most deliberate opinions and our best feelings, we should have regretted it indeed on your own account: but we should also have been indignant that our Professor chose to take advantage of his situation, to produce a distaste of our proper studies, and to deprive

us of those reflections which support us through a painful profession.

Your younger colleague in the chair of the College of Surgeons thought it unbecoming to restrain the expression of opinions, however abstruse and delicate, however remote from the subject of his lectures. He spoke of freedom ;—and of tearing the gown off his own shoulders, if he was to be controuled in any degree. By his surprise at the notice you have taken of him, it appears that he has not understood the difference betwixt the individual and the office he holds. If he had justly estimated the value of the gown at the time he received it, he would better have comprehended the motive of that attention he has met with. He stood as the organ of the College; and it is too much to expect that those who came there to support him as the Professor of the College, should be brought to sanction, by their presence, doctrines which they know to be founded in error.

This is now the third time that I have seen the influence "of lean translations out of French," upon our students; and it has appeared to me to subdue, in a remarkable manner, their love of the works of nature; and to cool that enthusiasm which should belong to their peculiar studies, and their time of life.

They know not how much they lose by being seduced from a just philosophy, in favour of such speculations, the offspring of vanity and seclusion; and leading to a stupid and sterile materialism. I would desire no other guard upon the mind of a youth against the infection of those notions, but that he should study some department of natural knowledge deeply: not in men's written opinions, which are so often distorted by passions and obscure motives, but in the object itself, in nature; above all, I would recommend to his observation the structure of animal bodies.

The human body, though deprived of what

gave it motion and feeling, is still a plan drawn in perfect wisdom. The student who comes to the examination of it with right notions, will think he is admitted to a great privilege; he opens, as it were, the volume of nature to see things near at hand. With men of talent the interest increases every day. Those of a different stamp, and who possess neither just feelings nor good taste, must treasure some of the sentiments and expressions of your colleague with particular satisfaction.

A man possessed of that humility which is akin to true knowledge, may be depressed by too extensive a survey of the frame of nature. The stupendous changes which the geologist surveys, the incomprehensible magnitude of the heavenly bodies moving in infinite space, bring down his thoughts to a painful sense of his own littleness: to him "the earth, with men "upon it, will not seem much other than an "ant-hill, where some ants carry corn, and "some carry their young, and some go empty,

"and all to and fro, a little heap of dust *." He is afraid to think himself an object of Divine care. But when he minutely regards the structure of his own body, he learns to consider space and magnitude as nothing to a Creator. He finds that the living being which he was about to contemn, in comparison with the great system of the universe, exists by the continuance of a power no less admirable than that which rules the heavenly bodies. He sees that there is a revolution, a circle of motions no less wonderful in his own frame, in the microcosm of man's body, than in the planetary system; and that there is not a globule of blood which circulates, but possesses attraction as incomprehensible and wonderful as that which retains the planets in their orbits.

The animation with which you have answered opinions on subjects which regard the

* Bacon.

happiness of others, and the mildness with which you have noticed criticisms on yourself, afford an excellent example.

I consider it the peculiar happiness of a life such as yours has been, which private virtue embellishes as much as those services you have done to Science, that you can raise your voice against false opinions without a suspicion of any motive but the most generous and pure; and that you can exert your powers on such a subject to crown all your former labours.

I am, my dear Sir,

With every Sentiment of

Friendship and Esteem,

Very truly yours,

CHARLES BELL.

Soho Square, April, 1819.

AN

ESSAY

ON THE

CIRCULATION OF THE BLOOD.

PART I.

AN Essay on the Circulation of the Blood conveys to most readers an idea of presumption in the author; as if he were about to put a rude hand on the honours of our great countryman. To others it gives only an impression of utter hopelessness of useful results. But in giving instructions in the first principles of that profession, which is to be the business of their lives, to men who have been accustomed to a just method of reasoning on

subjects of philosophy, the Author has felt in some degree humbled in entering on the circulation of the blood. He has been obliged to expose to them the want of just principles, and the substitution of mere authorities, in explaining by what force the blood is circulated. After many years he still finds the matter encumbered with difficulties.

The object of this Essay is to shew that, in one of the most common subjects of physiology, we give our assent to what we do not perfectly comprehend. We acknowledge the circulation of the blood, without attempting to remove those objections which every intelligent student is capable of offering to it. It is my purpose to state those objections; and to offer to my reader the course of reasoning with which I have met them when treating the subject in my lectures.

I shall not enter at present into an investigation of the whole of this curious subject,

but confine myself to such points as appear particularly to require illustration.

If I am performing an operation on the living body, a stream of blood is thrown out from the exposed surface;—an artery, the calibre of which is not greater than a pin, will throw out a stream of blood to the distance of three or four feet. Upon injecting the vessels of the dead body with water, whatever may be the force with which I use the syringe, the fluid may be made to ooze from the orifice, but not to rise in a jet.

While the blood is flowing in this powerful jet from the vessel of the living body, it may, by an application of styptics, be arrested in the open mouth of the artery. No such effect can be produced on the dead body: although the water only oozes from the orifice, no application will stop it. It therefore appears that there is a facility given to the course of

the living fluid in the living vessels that is lost in the dead tube. Simple and obvious as this fact is, the neglect of it has given rise to the most remarkable discrepancies in the observations and conclusions of learned men.

How are we otherwise to account for Borelli saying that the heart has a power of 180,000 lbs. and Keill that it has a power equal only to eight ounces? How are we to account for Senac estimating the pulsative power of the crural artery to be sufficient to raise a body of 400 lbs. while a very few pounds weight, placed on the artery, will certainly close it against the entrance of the blood.

As the blood flows uniformly and without pause in the veins, and as, at most, two powers only are operating on the column of blood, that of the heart, and that of the arteries, it is affirmed that the power of the heart's contraction reaches through the whole extent of vessels.

Further, Haller has asserted, that the impulse from the contraction of the heart moves the blood onward through the extremities of arteries the sixth part of a line in diameter. And in all the length of an artery, (as Senac observes,) the blood is seen to flow with greater impulse during the contraction of the heart, and with diminished force during its diastole.

Here then is the first objection which a student, who has been accustomed to reason philosophically, will start to the circulation of the blood. He sees that the arteries and veins are divided and multiplied to an incalculable degree of minuteness; that the blood is every where, and every where moving with rapidity. He recollects how the attraction operates betwixt the fluids and the solids spread out in contact, upon a surface many thousand times the extent of the surface of the body. The question at once occurs to him what power must the heart possess to propel the

blood against this influence? He turns to the heart; and, although he cannot calculate the power of the living fibre, he sees that the mechanical checks which direct the blood, are so weak in texture, as to be unable to bear the weight even of a few pounds or ounces; and, meeting with no answer to his enquiries, he already begins to look back with regret on those more philosophical studies which he followed at college.

The excellent Dr. Robert Whytt, in his enquiry into the causes which promote the circulation of the fluids in the small vessels, says that, "however easy it may appear to some authors to account for the motion of the fluids in the small vessels of animals, yet, whoever considers the resistance that a fluid, moving through the aorta and all its branches, must meet with from friction, and adds to this the mutual attraction and cohesion betwixt the particles of the fluids, and the sides

of the vessels in which they move, will not only see that there is some difficulty in this matter, but suspect that neither the force of the heart, nor the alternate contraction of the larger arteries, are sufficient to drive the fluids through the smallest vessels of the brain, testes, and many other parts of the body."

The ingenious Dr. Hales said, " If we could be so happy as to find a liquor of such a due consistence, as to pass freely through from the arteries into the veins, as the blood does in its natural state, then many curious experiments might in this manner be tried on several parts of the body."

From these celebrated men we have the statement of the difficulty; a difficulty which Dr. Hales encountered in all his experiments, however ingeniously contrived: the finest fluids could not be made to pass through the vessels of the dead animal, with the same free-

dom and velocity as the natural blood, though thick and viscid.

But is it true that the blood is viscid while in the living vessels? "Fluidity being," as Mr. Hunter has said, "necessary at the time of circulation, for its motion, distribution, and the easy separation of its parts," are we to suppose that this necessity is imperfectly fulfilled? The blood is the most subtle fluid in nature, and passes where our finest injections cannot be made to enter. And there seems reason to conclude, not only that the coagulation of the blood is an act of dying, but that the viscidity of the blood (as we see it in the fluid part out of the animal) is also a state incompatible with life and circulation.

Mr. Hunter has said that coagulation is "an operation of life: it is," says he, "particle uniting with particle, by the attraction of cohesion." I am forced to reject the first

part of the opinion, and adopt the latter. Coagulation is the act of death; by which the blood, hitherto preserved fluid, suffers an attraction of its particles, and becomes solid.

But it is not only by the attraction of the particles among themselves, that the new property is exhibited in the coagulum, it is also rendered manifest by its attraction to the sides of vessels, or to any contiguous surface. When an artery is cut in the living body, the hæmorrhage will be the more profuse the less the injury which the coats of the vessel have sustained; but neither a torn artery nor a bruised artery bleed.

If the limb be torn off, and the artery lie with its mouth open, the blood is coagulated and attracted to the coats of the vessel. If we injure a vessel while the blood is flowing, the blood is attracted to the mouth of the vessel, and the bleeding stops. Nip the blood vessel with the fingers, or with the forceps, and the

stream of blood is stopt; although in both these instances the mouth of the vessel be open.

This, it has been suggested, may be owing to the air admitted to the blood, to the change hence induced, and to new properties in the blood. But the same effects follow an internal wound, where no air can penetrate; and the coagulation and the attraction take place to a depth in the vessels which does not admit of this supposition.

These facts lead us to understand the difference betwixt the course of the blood through living vessels, and the flowing of water through dead tubes!—De Gorter has shown by experiments that, in order to push the fluid through the small vessels of dead animals, it is requisite that the fluid should fall from a height the double of what is required to equal the power of the heart. But did the fluid in the dead vessel spring as from a divided artery?

How much greater must the pressure be to make a small vessel throw out the fluid three or four feet? The phenomenon to be explained, without which we need not advance a step further in reasoning or experiment, is this,—from the living surface the blood rises freely, and in so small a jet that it is not perceived except when the blood is scattered on a white part of the dress. We cannot imitate this in the dead body. Again, when a living artery is giving out its blood in a stream, it will stop, and the blood suffer coagulation merely by injuring the vessel, although the mouth of the vessel remain open. When I witness the force with which the blood spouts from a wounded vessel, and when I consider the amazing number and minuteness of the vessels through which the blood is flowing at the same rate, and when I recollect the weakness of the heart, and the delicacy of its mechanism, I am brought to this conclusion. Philoso-

phers have been estimating a resistance which does not exist in the living frame. They have been in the old error; estimating the powers of a living body by the result of their experiments on dead matter. They have been estimating the course of the blood, (which has properties different from what it exhibits out of the body), as they would water conveyed in lifeless tubes.

Attraction is a term used to denote the power which draws bodies together; a property the most universal in nature; so that we may say every particle attracts every other particle. It is universal in dead matter, and necessary to the frame of the world. Without some such principle there could not be any motion; and, without it, motion would soon perish. Are we prepared to admit that this universal attraction of fluids and solids is *negatived* in the vessels of a living body,—that the great Architect, instead of accumulating forces to

overcome the vast resistance, has annihilated it, and rendered a smaller force sufficient to the end, and at the same time consistent with the delicate texture of our frame.

If this fact were satisfactorily established, how surprising and admirable would it appear, that the power which orders every thing in dead matter, by which all motions are begun, by which heavy bodies descend, light bodies float, rain falls, and vapour ascends, on which depends the flowing of waters, as well as the stability of solid fabrics, should be dismissed in this instance, as incompatible with the motion of the fluids, and consequently with the life of animals.

There is no other rational theory of secretion but that which supposes the chemical affinities of the blood to be changed by the influence of life in the smaller vessels. But that which modifies attraction may discharge it.

In this sense perspiration is a secretion; and, although arguments from analogy are dangerous, yet as the skin is a surface exposed to our observation and experiments, I shall apply my hypothesis to it.

If a glass tube be attached to the artery of a horse, the column of blood will suddenly rise in the tube, marking the force of circulation. If the animal be bled from another vessel, the blood will descend in the column, as the force of the circulation is enfeebled. When the blood is very low, and the animal dying, the surface will break out profusely in perspiration. It must be concluded that the force of internal impulse is not the cause of perspiration. Again, the most persevering efforts in injecting the vessels will never bring the water through the pores of the skin; and if a dead body be carried near the fire, the heat will not bring out one drop of fluid from the pores of the cuticle.

The examination of the pores of the skin, and a very simple experiment, will explain the phenomenon. The pores of the skin are tubes formed by processes of the cuticle which run deep into the cutis vera: they are capillary tubes. To exhibit the attraction in capillary vessels, I drew out a tube of glass to some degree of minuteness, and, breaking off the extremities, I dipped the end in wine; the wine rose in the tube by capillary attraction. I did this in the beginning of summer: on the commencement of winter I could not observe that the fluid was diminished. I was aware of the fact before I made the experiment; but I did not conceive that the attraction of the fluid for the tube would have so effectually resisted the attraction of a dry atmosphere. We perceive now how impossible it is that evaporation or perspiration can take place from the capillary tubes of the skin of the dead body. What then produces perspira

tion? Is it not a process of life which destroys the attraction and lets off the fluid? And is it not the same negative attraction which permits the blood to flow in the smaller vessels, without which they would retain the blood and stop the circulation?

I am inclined to think that this negative attraction is not without its influence upon the lengthened excretory ducts of glands, when there is a sudden flow; for example, of bile or saliva; and how the convoluted vasa deferentia testis are capable of discharging their fluids without it, I am unable to conjecture, when I consider their almost incalculable length, and the smallness of their cavity.

Surgeons have miscalculated the force of arteries almost as much as physiologists have mistaken the power propelling the blood. After reading the means of stopping hæmorrhage, would not one suppose that it required some particular force of body as well as energy

of character to stop the bleeding from a principal artery? What stuffing of clothes and pressing of one hand over the other! But what in truth is requisite to stop the pulsation in the most powerful artery? The pressure of the little finger is sufficient.

See with what slight force the blood moves along the veins on the back of your hand. Yet slight as the pressure is which will stop the flow of that blood in those veins, and slight, consequently, as the impulse of the blood must be, the blood will make its way back to the heart, throwing down all the impediments which you have calculated upon, and open the heart itself.

When Mr. Hunter laid bare the radial artery of a dog, and saw it contract, and, on cutting it across, saw the blood oozing only, and not flowing in a stream, he concluded that the obstruction of the blood was owing to the contraction of the artery. Can we agree to

this conclusion? Before this experiment was made, did not the blood flow rapidly through the capillary extremities of that vessel some fifty times smaller than the trunk even in its contracted state? Did the contraction of the artery bring it to the diameter of one of its own secondary or ternary branches? If it did not, it could not be the smallness of the calibre that stopt the blood when the artery was divided. It was the injury from exposure, and the dissection, and the consequent attraction that began to prevail betwixt the vessel and the contained blood. In such a case there is blood contained within the vessel; but that blood is coagulated. I have examined the radial artery of a man cut across by a musket ball, when the blood stopt spontaneously; I found the blood coagulated for an inch in the length of the artery.

When vessels are divided in a wound, and the blood flows at first impetuously, then

slowly, and when the red blood is stopt, and serum only passes, is it the contraction of the mouths of the vessels which retains the red globules? Let us follow the action out: presently pus mixes with the serum, and suppuration is gradually established. Globules larger than those of the blood are passed through the mouths of the vessels. Do the extremities of the vessels open and enlarge again? Then should the red blood flow afresh. Since this is not the case, we must conclude that, from the commencement of the retardation of the blood flowing from the divided vessels, the living coats are exercising an influence on the contained fluids. That the serum or the pus discharged are not merely the finer parts of the blood, percolating through the contracted vessels, but secretions, must be admitted, if we define secretion to be the blood changed through the influence of the living vessels. Without ac-

knowledging the agency of the living vessels on the composition of the blood, we shall not be able to account for the changes performed on this fluid in the body or the difference of experiments made in chemical vessels, and in the tubes of the living frame. But grant that there exists a mutual influence of solids and fluids peculiar to life,—then the step is easy to admit also, that the common attraction of cohesion is dimissed, and then our view of the subject of the circulation of the blood is consistent.

We have no longer to estimate a force of attraction too great for the heart to overcome, or to suppose forces in operation quite inconsistent with the weakness of our frame. The motion of the blood is facilitated by the abstraction of a power, instead of an enormous accumulation of force; and the attraction is resumed, as in the case of an opened vessel, when necessary to the safety of the individual.

It is solely in this way that we can explain the fact which is every day before our eyes: that the blood circulates through vessels so small that we require a microscope to see them, and yet that not a drop flows from the trunk of those vessels, when cut across, although it be fifty times larger in the calibre.

It is difficult to account for the stopping of the blood, when the artery is torn, without admitting attraction and coagulation to be consequences of the injury; and it seems to me impossible to account for the circulation of the blood through myriads of small capillary vessels, by the impulse of the heart, without supposing that there is in this living and healthy state of the body an absence of that attraction which is universal in dead matter.

In drawing this conclusion, I have proceeded on the idea, that if the principles of mechanism, or of chemistry, be admissible in the explanation of vital phenomena, they are

still incapable of solving our difficulties. When matter acquires life, it acquires also qualities and powers which it had not before, and which it loses the moment that life is extinguished. The extinction of life, so far as it relates to the body, being nothing but the loss of these properties. It may be argued, living powers are superadded to those of inanimate matter, when it is taken into the living frame, but none of its former properties are taken away and destroyed. Our blood gravitates, as we see, in the veins of the lower extremities: and when extraneous force acts upon a living body, it is as inert as when it is dead. But this relation of an animal body to the surrounding world, in respect to its gravity, corresponds with endowments even of our senses, which hold a relation with what is around us. Gravitation is necessary to the exercise of our senses as well as to the agency of the body. That the living body

can begin a motion ought, I think, to lead us to consider it in a very different light from the class of inanimate objects around us, although holding certain relations to those objects.

This reasoning may not be satisfactory. Some may still say it is not safe nor philosophical, to suppose that the matter of living bodies parts with any of its qualities; and that it is by addition, not subtraction, that the difference betwixt the two states of matter, the animate and the inanimate, is produced. But is there no such modification of dead matter? Does heat produce no difference in the running of fluids through tubes? Does electricity produce no such modification of attraction betwixt fluids and solids? In water contained in glass, metal, or wood, we have attraction in different degrees; but in mercury we have the contrary, for it is repelled by glass or iron.

Those who would object to my hypothesis

on the ground that it implies a violation of the laws of nature to suppose attraction destroyed or modified in living vessels, may find in these considerations a new reason of acquiescence, since even in dead matter we have instances of nearly a similar result.

I am aware that, in the second part of this Essay, I shall weaken my arguments as to the necessity of supposing the attraction betwixt the fluids and solids destroyed, since I am there to shew how powerful the arteries are in propelling the blood. But, admitting the power of the arteries, and, on the other hand, considering that the whole body is a tissue of tubes conveying fluids, I think we shall see reason to conclude, that if the fluids were attracted to the solids at all times, and through the whole system, they could not be moved forward. They must be like the sap in a tree felled at undue season, which remains stagnant until the timber rots.

PART II.

IN this second part I intend to treat of the action of the arteries, to shew what powers they possess of circulating the blood, and how they vary in activity, independently of the heart's action: at one time diminishing the velocity of the blood, and at another encreasing it in a remarkable degree. It is especially intended to draw the attention of the reader to the different forms of the arteries, and to explain the uses of this variety.

In what follows, as in what has preceded, I have endeavoured to discover the truth by the examination of the structure, and the observation of the phenomena of life, without torturing living animals. It is too common a

belief that, in physiology, experiments on living animals is the best and surest way of pursuing an enquiry, although it be certain that the supposed issue of experiments, is as much affected by the preconception, as the process of reasoning can be. The experimenter on brutes is not to be called a philosopher merely because he goes counter to the natural feelings of mankind; nor is he the more entitled to favour, that he gives a character of cruelty to the Medical Profession, thereby contracting its sphere of usefulness *. It is but a poor manner of

* That I have known the best and most virtuous men hold a different opinion, I must allow. But I have not been able to suppress the expression of my sense of this matter, that dissections of living animals attended with protracted suffering must be wrong. I can affirm, for my own part, that conviction has never reached me by means of experiments on brutes, neither when I have attempted them myself, nor in reading what experimenters have done. It would be arraigning Providence to suppose that we were permitted to penetrate the mysteries of nature by perpetrating cruelties

acquiring fame, to multiply experiments on brutes, and take the chances of discovery. We ought at least to try to get at the truth without cruelty, and to form a judgment without having recourse to torture. At all events, it is our duty to prepare for experiments upon living animals by the closest previous application of our reason, so that we may narrow the question, and make it certain that advantage shall be gained by the experiment.

Before entering on my proper subject, let me suppose, that an experimenter were to affirm that arteries do not dilate and contract, and were to propose to support his opinions by some fifty experiments on living creatures; I might be permitted to say to him,—

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which are ever against our instinctive feelings. I am, therefore, happy in believing that the examination of the natural structure, and the watchful observance of the phenomena of life, will go farther to give us just notions in physiology than the dissections of living animals.

Before you commit these cruelties, consider well, whether the object of which you are in pursuit be capable of demonstration in the living tube. Before you open the dog's or the sheep's throat, to look at the pulsation of the carotid artery, consider the quantity of blood sent out from the heart at each pulsation, estimate how much of this is expended on the elongation of the artery, estimate the number and length of the arteries, and put this question first to yourself,—Supposing that the artery dilates, is it in such a degree that my eye shall be able to distinguish it?

Not satisfied with this consideration, our experimental philosopher may yet suspend his hand, and reason thus:—All physiologists admit that the artery is elongated at each contraction of the heart: I may as well see, by examination of the coats of the dead artery, whether they be as yielding and elastic in their transverse direction, as in their length;

for if it prove to be so, and if it be true that pressure on fluids extends in all directions equally, then while the artery is dilated in length, it must suffer a proportionate dilatation in breadth.

But this philosopher having the knife in his hand, and the animal bound before him, is, notwithstanding, resolved to try the experiment. That is, he dissects the living animal; or, as it is more cautiously expressed, he lays bare the artery,—he sees it move with the systole and diastole of the ventricle of the heart,—but is absolutely unable to discover any change in the diameter of the vessel. Accordingly, he proceeds to prove by multiplied experiments on living creatures, that there is no dilatation of an artery, but only an elongation: yet, if he had reasoned on the matter, one, instead of fifty experiments, might have satisfied a reasonable man. As thus,—the carotid artery is six inches in length; half an

inch in diameter; it elongates at each contraction of the heart a quarter of an inch, and, of course, rises from its bed in a curve to accommodate itself to its confined place. Supposing that it dilates in breadth in the proportion of its elongation, ought I to see this dilatation? Dividing the inch into twelve parts, the length of the artery is seventy-two twelfths, the diameter six-twelfths. The motion of dilatation is to the motion of elongation as six to seventy-two; that is, twelve times less in degree. It further appears from experiments that the longitudinal motion of the carotid artery is in so limited a degree, that it cannot be at all times observed, but only in favourable circumstances. This being granted, is it reasonable to expect that a lateral motion of the artery should be observed, which is twelve times less in degree than the longitudinal motion which is caused by the same impulse, and is taking place at the same

instant of time? Thus, by omitting to question himself before questioning the animals under his knife, an experimenter may be led on to compose a volume of experiments.

There are sufficient reasons for believing that the arteries might propel the blood without the aid of the heart, if the uniform motion of the blood through the body were the only end of the circulation. We have reason to believe, that the heart belongs fully more to the lungs than to the body; and that its chief office is as a regulator of the circulation and respiration, preserving at all times a due relation betwixt them. Yet it cannot be denied that, constituted as animal bodies are, the heart has a powerful effect in propelling the blood. I have seen with the microscope, in cold blooded animals, the red globules of the blood propelled in pulses through the extremity of the vein; and, therefore, what is said by one physiologist of the blood flowing in a uni-

form stream from the extremities of arteries, and by another, of non-pulsating arterial extremities, is probably founded in error, or in inaccurate experiments.

But although it were admitted that the force of the heart is sufficient to carry the blood through the whole extent of the arterial system, it cannot be made to account for the inequalities which we observe in the force of the arterial actions. It cannot account for increase and diminution of secretion; for sudden and partial growth; for wasting and decay of parts, while the general body is vigorous. It will not account for an organ being plentifully supplied with circulating blood one hour, and the next left with a diminished quantity. Ought not these facts to be accounted for? In what state is our physiology when we must overlook these most common and obvious occurrences? To suppose that the heart is the only engine circulating the blood, or

even that it is the principal cause of the motion of the blood, must leave us in perfect astonishment when we see it ossified in its substance, or encumbered with a tumour which surrounds it wholly, and adheres to it on all sides ! *

Some physiologists have denied the iritability of arteries, they have neglected the evidence of anatomy, and the experiments of Mr. Hunter, and then have contrived clumsy experiments the better to correspond with their own fancies.

They have not enough considered that muscles will sometimes resist all stimuli but that of their proper agents. It would be too much to infer that the iris is not muscular, because

* See specimens in my Collection. But of this kind the most extraordinary case is that narrated by Mr. Allan Burns; where "the whole extent of the pericardium covering the "ventricles, and the ventricles themselves, except about a "cubic inch at the apex of the heart, were ossified and firm "as the skull."

it does not act upon being pricked! We see that the heart, after it is exhausted, and refuses to act whatever stimuli be applied, will contract when it is distended, because distension is its natural excitement. For the same reason an intestine will revive and act upon being distended with air, though it will not upon being pricked with needles. How then shall we contrive to excite the arteries in imitation of their natural stimulus? Or, supposing that we succeed in causing them to contract, how shall we perceive the action?

Hales having introduced a tube into the descending aorta of a living animal, and having raised a column of water which, by its pressure, might inject all the branches with a force equal to the heart, he cut the intestines in such a manner that the small branches were divided in the length of the intestine on the side furthest from the mesentery. He found that a certain quantity of water passed from

the cut vessels in forty-six seconds, while the same quantity of brandy took sixty-eight seconds; and on a second trial seventy-two seconds. He found, by the same mode of experimenting, that warm water, of the temperature of the animal, passed off much quicker than cold water from the pump. The natural inference from this experiment is, that the cold water and the spirits excited the muscular fibres to diminish the calibres of the vessels, and hindered the course of the fluid.

It may be seen at any time that the arteries of the turtle exert a muscular power. The viscera of a turtle having been sent from a hotel in Covent Garden to Windmill Street, the heart was still beating, and the intestines contracting. On attempting the injection of the aorta, it could not be dilated with all the force of the syringe. But, next day, when the parts were relaxed in death, when the intestines no longer exhibited their vermicular motions, the arteries

dilated under the pressure of the syringe, and admitted the injection freely.

Mr. Hunter's experiments appear to me conclusive. He proves that the arteries contract with a living power, distinguishable from elasticity, by its ceasing with life. It is not only offensive to see such authorities as Haller, Whytt, and Hunter disregarded, and their experiments and conclusions set at nought; but it is quite evident that Science cannot be progressive, unless we pay respect to the great men we have lost. I read in a French work where, with the utmost indifference to all we know through the labours of these men, it is written, that there are no muscular fibres in the arteries, not even in the aorta of an elephant; and that experiments have been made in every possible way to make these supposed muscular fibres act, but to no effect. The author then speaks with a soft indulgent regret of those who have been so

long deceived into a belief that arteries have muscular or irritable fibres.

Suppose that a physiologist were to put a pipe into the crural artery of a dog, and to force out blood and water at the corresponding wound in the vein, so as to make the jet rise from the wound, could it be affirmed on such proof that the heart is sufficient to the circulation, and that there is no need of muscular arteries? The plain answer is, that this is not all that is necessary to be accomplished through the apparatus of circulation. That artery into which the tube is introduced, can be made in the living body to partake of excitation; to deliver a greater or lesser volume of blood in a given time; to drive it with more evident pulsation, and further into the extreme vessels. How then can this partial increase happen if the power be in the centre only? How can an artery contract, how can it enlarge in a permanent manner, if the whole

power be given to the heart? How, if the artery possess elasticity only, can these phenomena be explained?

Water will find its level; nor does it signify whether the tube be straight or crooked, through which it descends; it will spout up to the same level with the water in the reservoir. But the ajutage or spouting pipe of a jet d'eau affects the motion of the stream. If the water rise from a small tube, it will not mount so high as if it were to pass through a large pipe supplied from the same reservoir. The larger the tube is, the water will spout the higher, provided the reservoir be kept full. To raise the jet to the highest possible degree, the ajutage pipe must not be turned up at a right angle, it must have a gentle easy curve. A polished round hole in the end of the pipe gives a higher jet than either a cylindrical or a conical ajutage; and a conical form is better

than a cylindrical form for it. It is also found that the longer the pipe is, the larger it must be in diameter to give full effect to the pressure of the water in the reservoir. Thus even when water is flowing smoothly from the uniform pressure of a reservoir, the friction or attraction makes a remarkable difference according as the tube is narrow or wide, straight or crooked, ragged or smooth. How much more must be the impediment, if the fluid be propelled in a jet, as the blood is sent out of the heart. There will, I am sure, be no objection made to the following experiments, on account of their simplicity. I filled an elastic gum bottle with water, and contrived that a heavy body should fall upon it.

1. The mouth of the short tube attached to the bottle was a quarter of an inch in diameter; by the weight falling, and the sudden force of compression, the jet of water was conveyed twenty-two feet.

2. Putting on a small tube of an inch and a quarter in length, and the twelfth of an inch in calibre, the same weight falling, projected the water five feet only.

3. Having attached a tube one foot in length, and the fifth of an inch in diameter, the jet was thrown out eight feet.

4. On twisting the same tube round a rod, the jet was carried four feet.

The water in all these instances was flowing from the mouth of the tube, before the weight was allowed to fall.

We see how impossible it would be for the heart to propel the blood through the arteries and veins of different calibres and curves, with the same force or with any regulated impulse. As the resistance of a fluid is greater when a body is moved faster through it, so in the curved and elastic vessels of an animal body, the resistance will be the greater, the more sudden the impulse.

We have now to consider in what the vessels of a living body differ from rigid tubes.

1. They are elastic. 2. The course of the fluid through them is by sudden impulse; not as by the uniform pressure of water in a reservoir. 3. They are in possession of a living property; a power of contraction greater than the power by which they suffer distention.

It is this latter property of irritability that subverts all those conclusions which we would draw from the laws of hydraulics. And it is by inattention to this (I should be inclined to say obvious distinction,) that physiologists have been led to conceive that a curved artery is merely a means of retarding the flow of blood.

Mr. Abernethy, in his lecture at the College, having drawn on a board what he conceived to be the course of the vertebral artery, and contrasted the crooked course of this with a straight tube, said bluntly, nobody but a

fool could believe that a curved artery was designed for any other purpose than for retarding the course of the blood. This, it must be confessed, is the general opinion, however it may be expressed.

Yet I believe that those who have adopted this idea, have their eyes closed upon the greatest beauty of the animal structure. I hope I shall be able to prove that the tortuous structure cannot be designed merely to retard the blood; and that this notion stands contradicted by the survey of the natural body and its functions, as well as by the effects of disease.

Let us attend, in the first place, to the difference in the form of an artery as it ascends and as it descends.

We find the arteries of the lower extremity less tortuous than those of the head. The arteries of the legs and feet run in a straight direction: we follow the arteries on the head, and, as they ascend, they turn upon them-

selves, and still mounting on the head, they become more and more twisted and reflected; and in the extreme branches, for example, of the occipital artery, the tortuous form encreases as they diminish in size, until they reach the vertex. As this fact cannot be denied, we may put it thus,—the arteries which carry the blood downward have less curvature than those which carry the blood upwards against the power of gravitation.

If a tumour grows upon the head, the artery of the temple, already tortuous, becomes three times more tortuous; it is seen twisting in its pulsation under the skin.

The arteries of the mamma in a woman giving suck, become remarkably enlarged, but more remarkably tortuous. The rami mammarii of the internal mammary artery I have seen twisted and as tortuous as the splenic artery. When the function of the gland

ceases, the arteries lose their tortuous form, and diminish in size.

The tortuous form of the artery prevails wherever the part supplied is subject to occasional encrease of activity. The arteries of the uterus, those of the testicle, those of the stomach and spleen, those of the penis which serve for erection, are tortuous beyond all other examples in the natural structure of the body. When we witness the sudden activity which prevails in some of these organs, or the rapid growth of others (for example the uterus during gestation); and when in that state we see the arteries becoming more and more tortuous as the activity or the volume of the body encreases, how can it justly he asserted that the tortuous form of the artery is for the purpose of impeding the course of blood!

If, according to the notions of some physiologists, the arteries possessed only an elastic power which aided the power of the heart as

the compressed air of the fire-engine does the forcing pump, what would the consequence be of a hole in the side of the vessel? Would not the elastic tube contract in consequence of the distention being diminished? The facility of escape having an effect upon the fluid similar to a diminished impulse. The reverse I hold to be the case in living vessels. An artery which is opened becomes the most active in the whole system while the blood continues to flow: hence bleeding from a distant vessel tends to stop the hemorrhage from a wound. But we require something more distinct than this assertion, and which shall amount to demonstration. The bleeding from the side of an artery would be too quickly fatal to give us time to make experiments, were we to attempt ascertaining the fact in this way: but may we not take the case of varicose aneurism, and call it an experiment without the cruelty and uncertainty which attend experiments. The vari-

cose aneurism is formed in consequence of the lancet in bleeding, transfixing the vein of the arm, and entering the artery; the blood of the artery finds access to the vein; and in time a channel is made betwixt the artery and vein, by which the blood has an easy conveyance out of the artery back to the heart, without passing through the extremities of the arteries and veins. This, in effect, forms a perpetual drain of blood from the artery, without weakening the system. The effect of this upon the artery is most remarkable; and must, I think, decide the question at issue.

" The trunk of the brachial artery is con-
" siderably *enlarged* all the way down the
" arm, and its pulsation so strong, that it is
" apparent to the sight; a little above the
" bending of the arm the artery makes a *re-*
" *markable serpentine turn*, which raises up
" the skin, and, by the force of its pulsation,
" looks as if it were a beginning aneurism.

" But, notwithstanding the size and the force " of pulsation of the brachial artery *be much* " *more considerable than in the other arm,* " the artery at the wrist is smaller, and its " pulsation much weaker." Dr. Hunter, Med. Observ. and Enq. vol. 2.

So very singular a fact was not passed over by Dr. Hunter: his remark is pertinent to our present enquiry. " Whence is it," he says, " that the artery is enlarged all the way down the arm? I am of opinion that it is somehow the consequence of the blood passing so readily from the artery into the vein; and that it will always so happen in such cases: that it is not owing to any particular weakness of the coats of the artery like that in true aneurism, but is rather such an extension as happens to all arteries in growing bodies, and to the arteries of particular parts when the parts themselves encrease in bulk." I shall not attempt to reason on this state of the artery

in varicose aneurism, until I present a similar consequence apparently from a different cause.

When the main artery of a limb is tied, the branches enlarge, and, by the inosculation of their extremities, they supply the lower part of the limb. But, during this change, their encrease in size is not so remarkable as their convolutions. While they enlarge, they become at the same time very tortuous.

To those who do not give themselves the labour of thought, this change seems very natural. They say you stop the great channel, and the lesser branches suffer the pressure, and are enlarged. But why does not the same take place in amputation? Why do these branches diminish in the one case, and enlarge in the other? For if the obstruction be the cause of their enlargement, then they ought to be most enlarged when most obstructed;

viz. in amputation. But the reverse is the case.

These facts, and many others, are inexplicable, unless we admit that the limb or organ supplied by an artery has an influence retrograde along the trunk of the artery that belongs to it. If we admit this, we can comprehend how the arteries finally contract and diminish after amputation by the absence of the influence. We can comprehend how the smaller vessels, taking the place of the trunk, and becoming the main supply, increase under that influence: and we then comprehend also, how in the instance of the varicose aneurism, the limb being imperfectly supplied with blood, (owing to the blood escaping by the side passage), it excites and solicits the trunk of the artery the more. So that, like the vessels of a growing part, the artery encreases in size and tortuosity. In this way of considering it, the hole in the side of the artery is the indirect

cause of the enlargement: it reduces the supply of blood to the fore-arm; and the fore-arm, in its influence on the trunk which supplies it, excites that vessel in a manner which Dr. Hunter has finely illustrated by the comparison with the artery of a growing part.

If any one holds his hand above his head for a few seconds, he will be convinced that there is an accommodating power in the arteries, to the difficulty of the circulation; the arteries will be felt pulsating to the finger ends; while, in the other arm, if it be in a horizontal posture, there will be no pulsation, nor unusual sensation perceptible. I must suppose that if this position of the arm were to be continued, the excitement which the vessels have to activity would be followed by a permanent change in their curvatures; and that they would become like the arteries of the head.

I request my ingenious reader to try this simple experiment; and, when he has acknow-

ledged the fact, to assign a reason for this change in the pulsation of the arteries of his fingers. He will, perhaps, say that it is the easy descent of the blood through the veins, which permits an easier discharge of the blood from the extremity of the arteries, and, therefore, a fuller contraction of their coats. Let him vary the experiment,—let him tie a cord round the arm, yet not so firmly as to stop the circulation, and hang the arm over the bed or sofa: here also the pulsation will be felt to be encreased, and from the same cause, viz. the difficulty with which the circulation is performed; an impediment is experienced in moving the blood upwards in the veins against the constriction of the cord, and the arteries increase their action to overcome it. I might state the matter in form of a question,—How is it that the circulation is equalized in the various postures of the body and limbs? How is it equalized during exercise, or during the

peculiar positions and continued actions of artizans, or during the existence of tumors and morbid obstructions? Is it not because the arteries exercise a power adjusted to the difficulties opposed to their activity?

In these examples I have stated a few of the difficulties with which they will have to contend, who bestow the whole power of circulation upon the heart; difficulties too, which they encounter who maintain that a tortuous artery is a provision for retarding the circulation of the blood. If a reason can be given why the blood should be retarded in the uterus during gestation, I shall be inclined to acknowledge all the other instances to have no force.

To suppose that the tortuous form of the artery is an accidental consequence of the pressure of the blood, would be to admit an imperfection in the design of the animal economy, and to harbour a notion which must prevent us from arriving at a just and true

estimate of the wisdom of the Creator. To consider the curve as design, and that the intention is to retard the course of the blood instead of increasing the force of the arteries; would be to suppose that mechanical obstructions were interposed, where, in fact, we find that there is a remarkable increase in the force of circulation, and a growth of the part rapid beyond example.

The very great confusion in which this subject, the circulation of the blood, has been left, is some apology for the easy admission of statements without proof. I have of late observed, with great surprise, the progress of an opinion that arteries are not muscular. Against the evidence of human anatomy,—against the analogy of the structure of inferior animals,—against the direct evidence of the senses, and on the mere ground that some chemist has thought he has discovered that there is no [redacted] fibrine in the tex-

ture of arteries; an opinion is taken up that the arteries are not muscular. It really appears as if men were desirous of altogether disavowing and overturning the labours of the great men of our own country who have gone before them. I cannot imagine men possessed of any relish for philosophical enquiry, or capable of pleasure in witnessing the exertion of genius, thus disregarding the labours of Mr. Hunter upon this subject.

The arteries are fibrous, dilatable, contractile; and a certain proportion of this contractility depends on the life, and ceases with it.

The velocity of the blood in the different branches of the arterial system is not the same. We find parts subject to an encrease of arterial action; we cut into an inflamed part, and the blood flows with more velocity, than in the natural state of the body. The exercise of an organ in its function will produce in it a fuller pulsation; its arteries will be more active, and

its blood will be driven further. If the uterus be examined during menstruation, it will be found to contain more blood, and to possess larger vessels. If the intestine of a dog be opened during the process of chylification, it will be much redder than at another time. How shall we account for the growth of one part of the body, and the decline of another, without admitting a partial encrease of action, and a partial diminution. How shall we explain the fact that we select a part that has been in activity to make a minute injection of its vessels, and that we cannot get the fluid to run into parts which have long ceased to be excited? As this partial action of arteries cannot be denied, we must also admit that it is their irritability, and their different degrees of excitement and growth which make the difference.

The power or effect of a natural action does not more depend on the muscular contraction,

its rapidity and force, than on the entire and ready relaxation of its opponent muscles. Without this, the limb would be rigid in spasm, not powerful in its effects. To come nearer in the analogy to the arteries, the relaxation of the heart is quick and complete in proportion to its natural and lively contractions. This we see in the heart, and must imagine to take place in the artery. The phenomena which I have described, as exhibited in the change of the artery, authorize the conclusion, that when an artery is excited, it is not only excited to rapid and unusual contraction, but also to sudden and complete relaxation in the intervals of that action. Grant that the influence is to promote relaxation as well as contraction, and every thing is plain. We then see how an artery admits of partial increase of pulsation; how the carotid system, for example, will partake of excitement: then comes a beating of the artery in the neck, a

pulsating pain of the temple, tension across the forehead, flashing of the face, animation in the eyes, flashing of light, and singing in the ears, while the hands and feet are cold. Then also the rapid growth of certain parts of the body beyond others can be explained when they encrease either in the natural or diseased state.

I shall here endeavour to state the argument shortly:

1. A tortuous artery is a more capacious artery. Hunter and Blumenbach, and others, have observed the diameter of the artery to encrease as it recedes from the heart; and this to be particularly the case in the vessels of the testicle and uterus, &c.

2. The artery being encreased in length and in calibre, it must contain more blood, and, therefore, act upon more.

3. If the muscular coat of an artery be greater the further it is distant from the heart, and if the muscularity of an artery be the

more the longer it is, so must the muscularity of a tortuous artery be greater than that of a straight one. The tortuous artery, therefore, acts with more force.

4. The further an artery is from the heart, and the nearer it approaches to its destination, the more it becomes under the influence of the part or organ; it is involved in the same nervous tissue with it; and subject to the same excitement. Therefore the power which the tortuous artery possesses, will be more under the influence of the part to which it goes.

5. The excitement of a hollow muscle does not consist merely in contraction, but in the free and perfect relaxation which follows the contraction.

The conclusion is, that a tortuous artery is more capacious, more active; that its activity is more under the influence of the part; that in its activity it dilates more freely, as well as

contracts more powerfully. In other terms, an artery, in proportion to its tortuosity, becomes less dependant on the force of the blood transmitted from the heart more on the excitement of the organ which it serves.

Returning with these views to our subject of tortuous arteries, it will not appear paradoxical if we say that the tortuous arteries both retard and accelerate the blood. For as, by experiments on dead tubes, it appears that a curve retards the course of fluids, and, as it as certainly appears, that in the living frame a tortuous artery is employed to encrease the force of the circulation, we may conclude,—

1. That when the tortuous artery is not excited, it retards.

2. That when it is excited, it accelerates the flow of blood.

And now looking to the organs of the human body which have an occasional remission of their activity, or which have an occasional

call for an increase of their function, we find that they are all remarkable for the tortuous form of their arteries.

1. The uterus in the interval of menstruation requires only to be kept alive; its vessels are in a quiescent state. But when the period returns, it swells, becomes more vascular, redder, an acme of arterial pulsation ensues, the menses flow, and then there is a subsidence of the action. If impregnation takes place, the excited action continues, the increased tortuosity becomes more obvious, and exhibits a singular congeries of twisted vessels. On delivery, the whole uterine system sinks again into its passive state.

2. The testicle in all animals is an organ of occasional activity. For the performance of its office there is required a sudden increase of vascular activity, while it is as obvious that this activity is not constant or uniform. It is an organ in a remarkable manner set aside

from the general tide and force of circulation, and is particularly inactive, unless excited.

Accordingly the vessels of the testicle are tortuous in an extraordinary degree. The artery comes from the aorta high in the abdomen, and runs a course at first straight, but, bye and bye, it forms certain curves; these turns, as the artery is traced downwards, become circles, and, as it approaches the testicle, it is so twisted as to form a body of a pyramidal shape, consisting entirely of convoluted vessels; and finally it dives into the body of the testicle, the branches being still tortuous.

3. I might here take the instance of the splenic artery, which is very remarkable for its tortuous form; and, it will be granted, that the parts to which it is sent, the stomach, the spleen, and pancreas, are occasionally excited; they are subservient to digestion; and, therefore, subject to temporary increase of action in the natural and healthy economy.

4. But, in truth, it holds universally that an artery becomes tortuous as it approaches its destination. Let us, for example, take two lymphatic glands, one seated near the heart, another in the thigh: in both, the branch of the artery which dives into the substance of the gland, advances to its destination by twisting and turning, and the curves, are the more remarkable the nearer it approaches to the gland. In the tortuous form of the arteries of the two glands no difference can be observed. Can we believe that the heart is made too powerful for the system, and that its impetus, even at the extremities of the arteries, requires to be counteracted by some mechanical hinderance?

Is it not obvious, on the contrary, that as an artery approaches the part to which it is destined, it becomes more under the excitement of that part? and that, in order to minister to the organ to which it is destined with-

out disturbance of the general motion of the blood, it is made tortuous, and, consequently, more muscular and active.

Comparative anatomy affords a confirmation of what is here advanced from the human anatomy. In brutes the muscles of the most powerful action, as those of the jaws of the lion and tiger, are supplied with tortuous arteries. Undoubtedly those arteries, by the superior power they are capable of exerting, serve to continue the circulation and the supply of blood to the fibres. For the fibres and fasciculi would otherwise, during their state of action, compress the vessels, and diminish that force of circulation, on the continuance of which the irritability or power of the muscle depends.

An attempt has been made to connect the tortuous form of the arteries which supply the muscles of the tardigrade animals with the slow motion of their muscles. But the circum-

stance to be explained is not the slow motion, but the long continued action of the muscles of these animals. The animal, with such a construction of the circulating apparatus of the muscles of the anterior extremity, will continue hanging by the branch of a tree during the whole night.

From these views of the subject, a question naturally arises,—What purpose does the tortuous form of the arteries of the brain serve, and what is the use of the *rete mirabile*?

The tortuous form of the vessels of the brain in some measure cuts the organ off, and secures it against the irregularities of the circulation; such, for example, as that sudden impetus which the blood in the arteries receives from the violent exertion of the trunk and limbs, and which will be explained in the third division of this Essay. At the same time, the tortuous form of the arteries

of the brain places them more under the influence of that organ, so as to measure the supply of blood according to the exercise of the function.

As to the *rete mirabile*, that convolution of vessels formed by the cerebral artery before entering into the brain of some brutes, it serves the same purpose, but in a superior degree. An animal with long legs, and, consequently, with a long neck, must have the circulation of the brain subject to sudden and remarkable changes, as it raises the head, or hangs it in grazing. By the rete mirabile the brain depends on its own excitement for its supply of blood, and becomes independent of the position of the head. An animal remarkable for the sudden changes from repose to activity, and for the highest degree of exertion when excited, (as the lion,) possesses, by this mechanism, both a means of saving the organ from the shocks and sudden changes to which

its circulation is exposed during its extraordinary activity, and yet a means of sustaining the vigorous circulation in the brain to a degree commensurate to the state of animal excitement.

Am I not borne out in saying that physiologists have overlooked the most obvious things, to encounter extraordinary difficulties? The tree of the arteries presents the most simple explanation of the forces circulating the blood. Here we see branches of different lengths and curves, and which, therefore, receive the blood from the heart with different degrees of force. But this inequality is corrected by their own powers: for as they possess a second power, and, as that power is greater in proportion to their lengths, they equalize the circulation, giving to every part, whether near to or remote from the heart, its proportioned force of circulation. If the part to be supplied be near

the heart, the impulse from the heart is greatest; if it be remote, the impulse from the arterial contraction is greatest.

Again, in supposing that the force of the heart moves the extremity of the toe loaded with a hundred pounds, they have forgotten that the weight of a few pounds will stop the pulsation at the groin. If they had thought of the tying of an artery with a hair, or of the compression of it with the point of the finger, or of the delicacy of the valves and coats, would the force of the heart ever have been estimated at one hundred and eighty thousand pounds? The calculation of the force exerted by the heart in raising a weight placed upon the foot has been a favourite theme: and if we could admit that the power of the heart was equal to the effect, we should be forced perhaps to admit the extravagant conclusions which have been drawn from this experiment. But it is altogether a delusion. I shall first state the fact and the form of the

argument. When one leg is twisted over the other knee, and the foot is held suspended, a distinct pulsation is perceived in the toe, and the toe will pulsate and rise, although the foot be loaded with a hundred pounds. Now here the whole power of the heart cannot be exerted, for its force is divided to the head, to the arms, to the legs; so that, at the most favourable calculation, it is a fifth part of the natural force of the heart which moves the leg. In the next place, the leg is here as a lever of the third kind: the moving power is the artery behind the knee joint; it moves the weight upon the foot in proportion as its distance from the centre of motion in the knee joint is less than the distance of the centre from the weight. How much, therefore, must the weight of a hundred pounds be encreased in its pressure upon the pulsating vessel? Yet the vessel throws it up! the heart distending the vessel, with but a fifth part of its

power, throws up this accumulated weight!! truly then the force of the heart is beyond calculation. What must it be at the aorta? What can withstand it? This consideration alone should have led to the re-examination of the presumed data, the grounds of this reasoning.

First, then, we find that at most a few pounds will stop the pulsation of the crural artery at the groin. In the next place we find, that if the knee be supported upon a small pad placed betwixt the ham-strings, or if the knee be suspended by grasping the condyles of the femur, there will be no pulsatory motion of the foot, although the leg hangs at right angles with the thigh; farther trials will show that the fleshy bellies of the muscles of the calf or ham must rest upon the knee to produce the effect contemplated. If the experiment be made with the arm, the same circumstances will be observed: when we rest

the elbow on the table, and bend the fore-arm to a right angle with the arm, no pulsation will be seen; but if the flesh of the arm rest on the back of a chair, so as to compress the veins, and make the bellies of the biceps and triceps press against the support on which the arm is extended, then the hand and fingers will pulsate like the foot in the other instance. But if the arm be made to press in such a manner that the trunk of the artery is compressed, and not the bellies of the muscles, then there is no pulsation observable in the hand and fingers. It follows, that we are wrong in supposing the motion of the foot is owing to the popliteal artery changing from the curved towards the straight line; or in supposing that it is the attempt of the current of blood to force itself against the compressed artery. It is not the popliteal or the brachial artery which throws up the limb; it is the pulsation of the smaller arteries

within the fleshy substance of the limb, that produces the motion.

This fact leads to a very different view of the subject; instead of estimating the power of the heart, we are induced to look to the arteries. The experiment being accurately made, will also prove that the pulsation of the carotids, and the motion of the foot, are not synchronous. If we divide the time of the pulse into three portions, we shall have the pulsation of the carotid in the beginning of the first period, the pulsation of the foot in the beginning of the second period, and the recommencement of the pulsation of the carotid in the end of the third period. This farther countenances the belief, that the motion of the foot proceeds from the smaller branches, and not from the pulsation of the trunk; a certain portion of time being required before the impulse from the heart reaches the extreme arteries. But a farther question

arises here: when the impulse is conveyed along the artery, from the heart, is it assisted by the contraction of the artery, and is there a succession of contractions in the arterial coats from trunk to branch, and bearing an accurate relation to the time that this impulse is received. If this be the case, then must the pulsation in the extreme arteries be not solely that received from the heart, but an accumulated arterial impulse also.

But whether this be admitted or not, if it be granted that each of the small arteries possess a certain degree of power, however small; yet when we enumerate the branches of an artery in the belly of a muscle, and calculate the amount of their combined forces, we shall discover a source of power, commensurate to the effect we have witnessed in the elevation of the foot, even when loaded with an additional heavy weight.

PART III.

When we have persuaded ourselves that we have arrived at some just notions of the powers circulating the blood, and have in imagination placed the frame of the human body before us, and contemplated the various results from the circulation of a living fluid: our conception of these wonders is imperfect, until we see the body in activity, and witness the effects upon the blood, of the change from repose to exertion.

The instant that a man becomes animated, or starts into exertion, the motion of the blood is thrown into disorder. There is no longer the measured activity of the heart, and

the gentle and equable motion of the lungs. The whole vital organs suffer the nature of a revolution. Is this an error or an imperfection in the frame-work? Far otherwise; out of this agitation, and seeming irregular violence, come additional means for sustaining the activity of the body. It is like those changes in nature, storms and tempests, and extremes of heat and cold, which seem the forerunners of misfortune, but which remove whatever is stagnant and noxious; preserving all nature in healthful activity.

The valves of the veins are provided for the exercise of the body; through them the pressure of the muscular frame-work, when employed in walking, running, leaping, or any sort of exertion, becomes a power additional to that of the heart and arteries in circulating the blood. While the veins are tubes, conveying back the blood which was sent out by the arteries from the heart, they

are, from their capacity and their numbers, also reservoirs of the blood which moves through them languidly. The veins are compressible by the muscles: this compressibility is so far from being an imperfection in the apparatus of the circulation (an opinion too hastily received), that it is attended with the most happy result; since through this effect solely, there is ever preserved an equality betwixt the force and rapidity of circulation and the muscular exertions.

Without the valves of the veins, which hinder the blood from moving retrograde, the pressure of the muscles would not effect this purpose of throwing the blood in encreased quantity upon the heart; the blood would be forced by exertion to the extremities, instead of towards the centre of the circulation. The observations of those who preceded Harvey went thus far; and Fabricius distinctly says, that the valves of the veins

were to prevent the blood from being forced outwards upon the extremities during exertion. They can bestow no additional activity, they only direct the impulse received from the muscles of the extremities towards the heart.

The heart assumes an activity proportioned to the blood which it receives; and the lungs, always in sympathy with the heart, partake of this activity and the respiration is encreased. The office of the lungs is to render the blood capable of supporting the life of the body, and in an especial manner the irritability of the muscles; now the motions of respiration are in proportion to the quantity of blood which has been compressed from the veins, and placed under the more active operations of the heart and arteries. It is thus that an activity is given to the circulation, and consequently the means of supporting the irritability of the muscles, in the proportion to its expenditure in exertions.

The circle of operations is in this succession; the muscles compress the veins; the heart is distended with blood; the lungs are excited by the state of the heart, the activity of the circulation and the respiration is thus promoted; and the effect is, that the circulation in the muscles is encreased, and their irritability thereby supported.

The action of the muscles has not only an influence in sending back the blood to the heart, but also in accelerating the flow of blood outwardly through the arteries. In performing an operation on an infant, or trying to suppress a hemorrhage from a drunk man, I have witnessed, with surprise, the additional force given to the jet of blood from an artery during the moment of exertion.

It is fortunate that we can have recourse to the account of experiments made by a man of veracity, instead of repeating hateful experiments on dying animals. When Hales was

attempting to estimate the power of the heart by attaching glass tubes to the arteries of a horse, and admitting the blood to rise into the tube, he observed that an occasional variation took place in the length of the column of blood; and this not attributable to the force of the heart, but to the exertions of the creature. He saw, even in the moment of its expiring, that the blood rose remarkably in the tube; and that on stopping the nostrils of the animal, the blood rose five inches; that it rose considerably and suddenly on the animal drawing a deep inspiration.

Hales observed accurately, but he drew a wrong conclusion. He thought this additional rise in the column of blood was owing to the dilatation of the lungs, and the greater freedom with which the blood passed from the right to the left side of the heart. On the contrary, we know that, during a struggle, there is a greater difficulty in the circulation

through the lungs. The true explanation of this effect, must be derived from the observation of the manner in which the heart and great vessels are guarded, by the tension of the membranes which are around them; and which tension is encreased in a remarkable manner during the violence of corporeal action; without which, indeed, the heart would be overpowered by the blood sent in upon it; and by which the additional force of the abdominal muscle and diaphragm, is still employed in accelerating the blood in the course in which it ought to flow.

In this review of the forces circulating the blood, by giving to the vessels, and to the membranes surrounding the heart, their due importance, the Author has somewhat diminished the value of the heart's action, and reduced it to the regulation of the general current of the fluids, and the action of the lungs

in connection with the circulating system. When we reflect that the blood of some creatures circulates without a heart, and see acephali born without a heart, yet fully nourished, —and when we see the aortic system of fishes removed almost out of the influence of the heart, —and when we see that the heart of all animals is placed in juxtaposition, and in accurate sympathy with the lungs,—it is impossible to refuse assent to the proposition, that the arteries possess the chief power in circulating the blood through the corporeal system; and that the heart is rather the regulator than the prime and efficient cause of the circulation. And by this it is not only meant that its state of excitement and activity commands and draws after it the motion of the blood generally, but that it regulates the actions of the lungs, in exactly according with the state of the blood and the necessities of the system.

The Author has shewn that the irregularity

in the demand of remote parts for blood cannot be answered by the acceleration or diminution of the heart's action; that the principal organs of the system have a provision for that partial encrease of activity in their vessels which does not disturb the general economy, nor call for the action of the heart. He thinks he has shewn that the object could not be effected by the encrease or diminution of the heart's activity; and that if the endowment and vital properties of the organs were entirely dependent upon the general force of circulation, and not on the capacity of their own system of vessels, to encrease or diminish the force of the blood: life would be held by a still more precarious tenure than it is: the vital action would interrupt the general system, and the agency of passion, and mental, and even corporeal activity, would disturb the economy of the organs essental to life.

For entering on this subject he has already

offered the apology, that he felt himself obliged to do so by the nature of his daily occupations. But surely there is another and better reason in the nature of the subject itself. To ascertain the difference between fluids moving in pipes, and according to the laws of extended nature, and the circulation of fluids in the vessels of living beings, must be an important part of science. It is interesting to him who loves to take an extensive survey of nature, and very important to the student who is about to devote himself to the survey of animated nature, to perceive, by these proofs, that there are new principles and new laws to be studied. By the novelty of this enquiry, to some, it may prove the occasion of opening those sensibilities to the works of nature which, by habit of inattention, have been lost to things seen in the more familiar path of existence. To contemplate with the microscope the circulation of the blood in minute vessels,

makes the head giddy, so surprising is the rapidity of the globules of blood; and on raising the head, and calmly considering the matter, the surprise does not cease: we have surveyed a new world, where the velocity and seeming impetus have no sufficient cause, and to which the laws of things hitherto familiar do not extend.

THE END.

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POWERS

OF THE

ROOTS OF THE NERVES,

IN HEALTH AND IN DISEASE.

LIKEWISE,

ON MAGNETIC SLEEP.

BY

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M.DCCC.XXXVII.

POWERS

OF THE

ROOTS OF THE NERVES.

The immediate cause of my putting forth the following exposition of my views of the functions of some of the most important parts of the nervous system, has been the publication of Mr. Grainger's treatise on the structure and functions of the Spinal Cord, followed by, to me, some very interesting conversations which, after my reading that work, took place between Mr. Grainger and myself. I find that, in my former writings, (in which I have probably, too much studied conciseness,) I have failed in stating my opinions upon the points referred to, so clearly as to prevent their being misunderstood. I have, therefore, determined to re-state them in a more perspicuous manner, and apart from the inquiries with which, in my former works, they are mixed up. I may mention that I should probably have hesitated to intrude a recapitulation of the following views of these subjects upon the

physiological reader, but that I believe them to be susceptible of various practical applications; and to form the basis of that true theory of the nervous system, which several sensible men in my profession have recently told me they anticipate will be discovered before long.

I wish the reader to conceive that the nervous system of man, and of all vertebral animals, consists of the following parts:—

First, of a DOUBLE CORD of nervous substance, giving origin laterally to pairs of nerves.

The longest and thinnest part of this double cord is in the vertebral canal, and is called the SPINAL CORD.

The rest of the double cord is in the cranium, the part of which immediately continuous with the spinal cord is called the MEDULLA OBLONGATA; the part beyond that, being its higher or anterior terminus, is called the TUBERCLES, THALAMI, and STRIATED BODIES.

Nerves rise in pairs from the whole length of the double cord. Those which rise from the spinal cord are called SPINAL NERVES; those which rise from the medulla oblongata, tubercles, and striated bodies, are called CEREBRAL NERVES.

The nerves, in reference to consciousness, it was discovered by the independent researches of Sir C.

Bell, M. Magendie, and myself (each having contributed his separate share to the result,) are of two kinds, and of two kinds only, one SENTIENT, or of sensation, the other VOLUNTARY, or of the will. The consideration of the other functions of these nerves, and of the nature and office of the sympathetic nerve, I exclude from the present essay.

Secondly, of hemispherical masses, called the CEREBRUM and CEREBELLUM, which are placed on the upper parts of the double cord,—that is to say, on the medulla and tubercles, of which they are, in one sense, direct productions. These parts do not originate nerves; but it should be remarked that the tubercles and striated bodies, in addition to originating nerves, are implicated in the structure of the cerebrum.

Now, not to go further back than the time of Whytt, for the dawn of sound knowledge upon the physiology of these organs, it appears that the opinion then prevailed that the lower mental endowments were, in some sort, so generally diffused through all these organs (and, indeed, through other parts of the body,) that when an animal is cut in pieces, each portion for a time retains a certain share of consciousness.

"We shall recite," says Whytt, "a few experiments and observations, from which we are led, by analogy, to conclude that the motions of the separated parts of animals are owing to the soul,

or sentient principle, still continuing to act in them*."

"A frog lives, and moves its members, for half an hour after its head is cut off,—nay, when the body of a frog is divided in two, both the anterior and posterior extremities preserve life and a power of motion for a considerable time."—(Quoted from Kaau.)

"The bodies of vipers not only move two or three days after they have been deprived of their skin, head, heart, and other bowels, but are also manifestly sensible of punctures, by means of which they may made to move with great vivacity."—(Quoted from Boyle.)

I select these from many passages in the writings of Dr. Whytt, in proof that that profound physiologist was well acquainted with the fact that, after decapitation, the body will exhibit for a time phenomena which resemble those of voluntary motion; and, in addition, that he thought *those motions proceeded from some modification of consciousness remaining in the mutilated trunk.*

I. The first step towards correcter motions consisted in the impression gaining ground, that when the head is cut off in vertebral animals, all sense, feeling, and volition, are instantaneously extinguished

* Works of Robert Whytt, M.D., quarto edition. Edinburgh, 1768, pp. 203-4.

in the body, and that the movements, which the body and limbs then exhibit, are purely automatic or mechanical. Dr. Hall and Mr. Grainger attribute the first recognition of this principle to Sir Gilbert Blane*. I remember at the period of my early medical studies, the point now before us was received as an established truth, the evidence for

* Dr. Grainger quotes the following expressions from Sir Gilbert Blane:—"These facts clearly show that instinctive, or rather automatic, motions may be exerted, without the intervention of the *sensorium commune;* and therefore without sensation or consciousness." I imagine, however, that Sir Gilbert Blane's views were only twilight anticipations of the truth. Contrast, for instance, with the preceding the following passage:—

"I took a young cat a few days old, and divided the spinal marrow, by cutting it across at the neck. The hind paws being then irritated by pricking them, or by touching them with a hot wire, the muscles belonging to the posterior extremities were thrown into contraction, so as to produce the motion of shrinking from injury. The same effects were observed in another kitten, after the head was entirely separated from the body. In repeating this experiment, I found that when the spinal marrow was cut through between the lumbar vertebræ and os sacrum, the posterior extremities lost their irritability, but the part below it, the tail, retained it. It might, therefore, be said, that the spinal marrow below the division served as a sensorium; but it may be answered, that when its head is cut off, its irritability remains, as appears by the motion of the ears, when pricked or touched with a hot wire; and as the extremities are also irritable, it will not be said that consciousness and sensation exist in two separated portions of the same body. *Nor can it be admitted that sensibility and consciousness may remain in the head after separation.*"—(Croonian Lecture by Sir Gilbert Blane.) 1788. *Phil. Trans.*

which then and now commonly given, and certainly of a most conclusive nature, was and is, that in men, whose spinal marrow is torn through, or cut through by accidental injuries, the lower part of the body, or that supplied by nerves which arise from the spinal marrow below the point of division, is totally without feeling, and cannot be moved by the direct volition of the patient.

11. Supposing it, then, to have become gradually received, that when the spinal cord is divided, the part posterior to the division has no further participation in consciousness, the next great step was made by Magendie, whose words I shall use to describe his progress in this wonderful inquiry. I beg to say, that—without entering into the question, whether this eminent physiologist has, or has not, exceeded the *justifiable* limits (if any are so) within which the lower animals may be put to suffering, to verify important physiological conclusions, else not verifiable,—all M. Magendie's statements as to matters of direct observation, and many and surprising they are, are implicitly to be relied on. I am, indeed, sometimes disposed to draw conclusions different from those adopted by him, but his statements of facts are thoroughly accurate and faithful.

"If you remove, by successive slices, in an animal with the cranium opened, all the parts of the cerebrum, then the optic thalami, then the

entire cerebellum, in such a manner that the last section shall be anterior to the fifth nerve, the animal continues to have perception (*conscience*) of all the sensations which have their seat in the face, excepting vision. It continues to be as vividly affected by sounds, smells, tastes, punctures of the face, as if it had experienced no further injury than the loss of blood attending the opening of the cranium. If, for example, you pluck a hair of the whisker, or apply a pungent acid to the nose, it tries with its fore feet to disembarrass itself of the cause of its pain, as if it were unmutilated. Respiration and circulation take place; the movements of the body are not more interfered with, than if the cerebellum alone had been removed. And as long as the continuity of the fifth pair with the segment of its insertion is not interrupted, these phenomena continue. I have seen them in hedge-hogs last more than two hours."

"The sensibility of the trunk andlimbs has, besides, suffered no alteration. The animal cries and agitates itself, endeavours to withdraw itself and to defend itself, when a toe or the sole of the foot is pinched, equally as when the lips or the nose are injured."

"These impressions on the trunk terminate (*aboutissent*) thus at the same point with those of the face, and of the organs of the senses except the eyes."

"And this place of reunion of all the sensations of the body, except sight, is the segment of the lobe of the fourth ventricle (medulla oblongata). For a section of the cord behind the medulla oblongata, deprives the animal of the perception of all impressions made upon its trunk; which may yet be thrown into convulsive action, without the animal's perception being excited, or its uttering a cry; while, during these convulsive actions of the hinder part of the body, pricking its face causes it to do so."

"But the whole extent of the medulla oblongata is not equally the seat of this perception. If behind the insertion of the fifth pair, and *on one side only*, the upper fasciculi of the medulla oblongata are divided, the phenomena above described are not reproduceable on that side, the perception of all the sensations before excitable continuing on the other; on the other hand, dividing the lower part of one half of the medulla oblongata, does not prevent perception of sensations excited in the face. Thus the intermediate parts of the medulla oblongata are neither conductors (to the other side), nor are they the seat of perception, which, besides, seems to have two centres (one for each half of the face and body). For the division of the right half of the medulla oblongata allows perception to remain on the left side, and vice versâ. After thus looking

to the transverse limits of these centres, we may examine the longitudinal limits. *The limit in front appears to be the roots of the fifth pair**."

The conclusions of Magendie and Desmoulins, drawn from the preceding and other similar facts are, that "It is in the medulla oblongata that in all (vertebral) animals the perception of all sensations, except that of sight, takes place; and in addition, in which resides, in reptiles, the faculty of determining or of willing†."

The reader will not fail to observe the singular superiority which is in this passage attributed to the medulla oblongata of reptiles. The following quotation throws a clearer light upon this supposed difference between reptiles, and other vertebrata.

"In frogs, toads, vipers, and snakes, not only does the perception (*la conscience*) of sensation survive the destruction of the cerebrum and cerebellum; but the animal likewise continues to determine and to will, which a mammiferous animal (so mutilated,) does not do. The faculty of willing and of determining resides then, at least in part, in the medulla oblongata in all reptiles, since it manifests itself when there is no longer a brain. This force evidently has not the same seat in mammalia. But since these two forces have not the same

* Magendie et Desmoulins, des Systèmes Nerveax des Animaux à Vertèbres, p. 560.

† Magendie et Desmoulins, op. cit. 575.

locality, and that one may exist without the other, they are not identical. To will and to determine, are then another (a separate) phenomenon, to having the perception of a sensation*."

III. Magendie thus had enunciated and experimentally shown the importance of that part of the medulla oblongata, in which is the ROOT of the fifth nerve, to the perception of all sensations, but those of sight, (in this exception he was wrong.) And I have not hesitated to attribute to him all the honour of this discovery; although other evidence had led physiologists to approach to such a conclusion. The evidence to which I allude, is that which the history of infants, born without either cerebrum or cerebellum afford: of these, the most satisfactory was communicated to the Medico-chirugical Society, by Mr. Lawrence; and some time anterior to Magendie's experiments, in an article in the *Edinburgh Review*, written, I believe, by the late Dr. Gordon, the general conclusion had been drawn from that case, which Magendie arrived at by experiment; namely, that the cerebrum, cerebellum, and tubercles, are not necessary to sensation.

In the second part of my "Anatomical Commentaries," (p. 138,) published in 1823, there further occurs this passage, showing a still closer approxima-

* Magendie et Desmoulins, op. cit. p. 566.

tion to Magendie's inferences. "From the history of acephalous infants, which have survived their birth for a short period, it appears that movements resembling instinctive movements, have been performed by human beings in which the cerebellum and the hemispheres of the cerebrum were wanting." I beg to observe that, by instinctive movements, I meant then and now, certain movements following sensation, and executed by the will. So (p. 136,) I have defined instinct to be "an arbitrary connexion between certain internal feelings, and the will to execute particular movements."

But the consideration of all these facts, and of Magendie's experiments, led me gradually to adopt a larger view, and as I conceive it to be, a juster conclusion*. I believe the segment of the medulla oblongata in which the fifth nerves arise, (or as I have here more loosely phrased it, the ROOT of those nerves,) to exert an influence, not only *downwards*, along the spinal cord, but *upwards*, likewise, towards the brain: I believe that the participation of every part of the nervous system in consciousness, depends upon its continuity by nervous substance with that segment. I believe, that the

* I pass over Magendie's mistake, as to the sense of smell not being destroyed, when the cerebrum is removed; and the distinction that he draws between the importance of the medulla oblongata in reptiles and in the higher vertebrata, which is unfounded, the same law, as I conceive, regulating both.

organs in which thought and reflection are seated, namely, the cerebrum and cerebellum, are as much dependent for the continuance of their functions on their continuity with the upper part of the medulla oblongata, as are those of the organs of sensation and volition which exist in the spinal cord.

The experimental facts, upon which I build these conclusions, are indeed variations only of Magendie's experiments, and they depend, in part, for their force in the argument upon analogical considerations.

a. The cerebrum and cerebellum being removed from a living frog, the mutilated animal was alive and conscious, drawing its limbs up under it, moving them all harmoniously, when disturbed, like the acephalous human infant.

b. The head being cut off from a frog, the body preserved its irritability, so as to move on the skin being irritated; but the limbs were not then moved harmoniously; nor did the animal gather itself up, as in the preceding instance, nor its parts appear to have a common principle of action. But the head appeared conscious. This was still better seen in the head of a turtle; it remained perfectly sentient, and capable of voluntary motion.

c. When, in the first animal, the upper part of the medulla oblongata was lacerated, the animal

became reduced to the state of the trunk of the second: consciousness had been extinguished.

d. When, in the head of the second animal, (or in the head of a decapitated turtle,) the upper part of the medulla oblongata was lacerated, all signs of sensation and life vanished instantaneously.

e. When, in a healthy frog, otherwise unmutilated, the upper part of the medulla oblongata (the ROOT, or place of origin, of the fifth nerve namely) was lacerated, both the head and body were simultaneously and instantly deprived of consciousness, at least of all movement that can be reasonably supposed to prove the continuance of consciousness.

Reasoning upon these facts, I am not disposed to conclude, with Magendie, that the upper part of the medulla oblongata is the seat of sensation, or of sensation and volition jointly; for, by the same process of reasoning, I must conclude, that the medulla oblongata is equally the seat of thought, deliberation, desire—to the manifestation of which by the cerebrum and cerebellum, the continuity of those organs with the medulla oblongata is not less necessary. Instead of adopting either of these conclusions, I think that the logical deduction from the facts which I have adduced, and others parallel to them, amounts to this only—*that the spinal cord has no part in consciousness, and that the cerebrum*

and cerebellum are equally excluded from their part in consciousness, when they are separated from the medulla oblongata. We may deduce that the concurrence of two organs is necessary to the performance of a class of functions, from evidence which may be totally insufficient to prove what part of those functions is performed by each.

IV. The next step consisted in ascertaining what powers reside in the different parts of the double cord, independent of the *vitalizing* influence of the medulla oblongata.

In the year 1823 I made this experiment (one of a series by which I determined the source of the actions of the iris in birds). The head of a pigeon being cut off, the upper part of the cranium was immediately, but without violence, removed with a sharp scalpel, the cerebrum and cerebellum were then likewise removed, and the medulla oblongata, leaving the tubercles only, which were in connexion with the eyes by means of the second and third nerves. (Now, the third nerves I had already determined to be the motor nerves of the iris.) I then divided the optic nerves;—when I found, upon pricking the end of the optic nerve adhering to the tubercles, that the iris contracted. *I had irritated the nerve of sensation* (now extinct,) *and the impression conveyed to the tubercles had excited there some change, which led to a motive impulse being propagated through the third nerve to the iris.*

I compared the result of this experiment made on nerves and their roots *on the cerebral side* of the medulla oblongata, with similar experiments made upon the nerves arising *on the other side* of the medulla oblongata, that is, from the spinal marrow, and I viewed them as one phenomenon. The general conclusion from these observations I stated in the following words in 1823:—"An influence may be propagated from the sentient nerves of a part to their correspondent nerves of motion, through the intervention of that part alone of the central organs of the nervous system to which they are mutually attached. Thus, in vertebral animals, in which alone the fact is questionable, when the spinal cord has been divided in two places, an injury of the skin of either region is followed by a distinct muscular action of that part. Again, if the brain is quickly removed from the head of a decapitated pigeon, excepting only the fore-part of the crura cerebri, together with the tubercle and second and third nerves, on pinching the second nerves the iris contracts."

In connexion with these observations, I perceived, and explained the law which regulates the origin of nerves. In vertebral animals, in mollusca, in the articulata, and in the higher radiata, "*the nerves of sensation and motion, which supply any given region in the body are derived from one and the same part in the nervous centre.*" The reader

will not fail to see the connexion between this and the preceding principle. By this it was established, that each segment of the body is supplied with nerves of sensation and motion from one locality in the double cord; by the former it was shown that, under certain conditions, an impression made upon a nerve of sense requires that segment alone of the double cord in which it rises to be perfect, to be able to determine through its means an impulse to muscular action, that is thence propagated along the motor nerve.

Another point, however, still was wanted—it was necessary to determine whether any connexion existed independently of the vitalizing segment of the medulla oblongata, between one segment of the double cord and another. The following extract from the second edition of my "Physiology," published in 1829, will show that I had then resolved this doubt. The last fact mentioned in it, which, taken with the preceding argument, amounts to proof, is contained in Whytt; but I happened to become acquainted with it first through an anecdote communicated to me by the Rev. Blanco White. "The cords, which unite the nodules in the nervous systems of invertebral animals, we may presume are intended to transmit reciprocally the influence of the different segments of the nervous system from one to another. The white fibrous strings, which form the outside of the spinal

cord in man and vertebral animals, have probably the same office. In the experiment upon a rabbit above described, the division of the spinal cord in two places produced three independent centres in the nervous system. If, in a snake, the head alone is removed, upon wounding the middle of the body the neck is raised, and bent towards the point at which the injury is inflicted."

So it was proved that the parts of the unconscious body for a while display a *certain degree of consent.*

V. The last step in the inquiry still remains to be considered. In a living healthy animal, are the voluntary muscles ever brought into action through the influence of one segment of the double cord, or of one group of segments, with the nerves arising therein, independently of the medulla oblongata, and of consciousness?

The following observations, bearing upon this point, I extract from my "Anatom. Comment." No. II. p. 17, published 1823; speaking of the muscles usually considered voluntary, I observe, "some, as the muscles of the trunk, of the limbs, and the like, are distinctly influenced by the will; in others again, as the muscles of the chest, and of the face in the expression of emotion, the influence of the will is not consciously employed, under ordinary circumstances, to produce their action; and a ques-

tion arises, whether this or some other principle stimulate the muscles on these occasions. Much may be advanced on either side. On the one hand, it is clear that *an influence, independent of the will, occasionally throws voluntary muscles into action, as appears in tetanus, and other spasmodic disorders;* and is shown remarkably in the physiological experiment of irritating the skin on the lower extremities, after the division of the spinal cord in the back, when the occurrence of action limited to the muscles of the lower extremities evinces that a connexion exists, independently of the will, between sentient surfaces and the action of voluntary muscles. I have varied this experiment by dividing the spinal cord at once in the neck and in the back, upon which three unconnected nervous centres exist; and the division of the skin in either part, (and especially at the soles of the feet in the two hinder portions,) produces a convulsive action of the muscles in that part. *The same influence may then possibly regulate the unconscious actions to which these remarks relate.*"

The hypothesis which I here advanced, and so far maintained the reasonableness of, is that which has lately obtained notoriety under the name of the reflex function of the spinal cord. It is based upon the facts, which I have before mentioned, and had observed and reduced to one theory in the year 1823. Dr. Marshall Hall, who invented the term,

"reflex function," has followed out the idea with great diligence, and has made several interesting and new experiments, showing *fresh* instances, parallel to my own published some years before, of the independent action of the segments of the double cord, and of their nerves.

To myself this hypothesis, when it originally passed through my own mind, offered no attractions. I thought that the muscular actions of the human body in health (excepting the vital motions of the viscera,) were better explained, by supposing consciousness to be at the bottom of all, than by supposing volition to regulate some, and a different principle others. I thought that this view accorded more with the simplicity of nature, more with anatomical structure, more with probability. The following passage from Whytt, which to my ear rings like true metal, expresses exactly the opinions which I have adopted.

"The necessity of air and aliment commences with our birth; and as we are excited to take in meat and drink by the uneasy sensations of hunger and thirst, which, as faithful monitors, never fail to warn us when these are wanted, but immediately cease upon the appetite's being satisfied; so, to prevent our danger of perishing through the the want of fresh air, there arises, unless the action of breathing be continually repeated, and new supplies of fresh air thus brought into the lungs, an

uneasy sensation, which may not be improperly termed the APPETITE of breathing.

"If then an appetite for fresh air be as natural to animals after birth as a desire for aliment, and if none ever thought of accounting either for sensations of hunger and thirst, or for the taking of food consequent on them, *merely from the mechanical construction of the stomach, gullet, and fauces*, without having recourse to the mind; why should we attempt to explain the action of respiration from principles purely mechanical, and deny the perception and operation of a sentient active principle to be the cause, which at first begins, and can after continue it? I should think that the analogy were too strong not to strike every unprejudiced mind*?"

"The objection against the mind's being concerned in the [vital and other]† involuntary actions, drawn from our not being conscious of its interposing for that purpose, is removed, by considering that many, even of the voluntary motions, are performed, when we are insensible of the power of the will excited in their production. Thus, while in walking, we either meditate by ourselves, or converse with others, we move the muscles of our legs and thighs, without attending to it, or knowing what we are doing. We are not sensible of

* Whytt's Works, 4to., p. 110.

† I have bracketed his single mistake.

the eyelids being kept open by the continued operation of the will; but yet, when drowsiness and sleep steal upon us, we find it requires a considerable effort to prevent the falling down of the *superior palpebræ.* The same thing is true of the action of the muscles which support the head. The most probable account of our ignorance of these things seems to be this; viz., that we not only acquire, through habit, a faculty of performing certain motions with greater ease than at first, but also in proportion as this facility is increased, we become less sensible of any share or concern the mind has in them. Thus, a young player upon the harpsichord, or a dancer, is at first solicitous about every motion of his fingers, or every step he makes, while the proficients or masters in these arts perform the same motions, not only more dexterously, but almost without reflection or attention to what they are about."

I am satisfied to suppose with Whytt, that instinctive actions are voluntary; that they proceed from sensation, and that they flow from the will, set in motion by a determined law.

So in deglutition, when the morsel reaches the root of the tongue, I prefer to think that the lingual branches of the glosso-pharyngeal nerve carry a sensation to the medulla oblongata, from which, by an instinctive law, a voluntary impulse is issued along the pharyngeal branches of the same, and of

other nerves, to the muscles of swallowing. I prefer this, and think it more likely, than that the process is automatic and withdrawn from the will, and the like.

For all *practical purposes* it is evidently the same, which conclusion we adopt, *when we have admitted, that in abnormal cases, there may be an action of the circle without sensation and volition;* and such I assumed to be the case in the extract which I have last given from my "Anatomical Commentaries," in p. 20 of this essay.

Having come to the conclusion, that sensation and volition, guided by instinct, regulated the *disputable* actions of the voluntary muscles in health, I followed out in detail the influence of the nerves of sensation in this function; I may give the results in a passage from the third edition of my "Outlines of Physiology," published in 1833, p. 219; the reader will see in it an extension of the original law, respecting the place of origins of nerves, which I had pointed out several years previously.

"I believe that the observation will be found to be correct, that nerves of motion take their rise from the same region or segment with those sentient nerves which transmit the impressions, by which their action is usually regulated. The correctness of this remark, as it respects the spinal nerves, will not be disputed. In the preceding figure, the third and fourth nerves, one of which

governs the motion of the iris, while both guide the eye to suit the wants of vision, are seen to rise near the optic; but the principle which I have laid down, is more strikingly illustrated by referring to the origin and uses of other nerves."

"We observe that the smaller portion of the fifth rises from the upper part of the medulla oblongata, close upon the greater portion; and we recollect, that the sense of pressure upon the teeth and gums, and of muscular exertion attending upon it, depends upon the latter, the muscular effort itself upon the former."

"We observe, that the large root of the fifth and the portio dura rise together; and we recollect, that the delicate sense of touch upon the eye and eyelids depends upon the first, and the action of the orbicularis palpebrarum upon the second; that the sense of touch in the nostrils depends upon the first, and the action of the muscles of the nostrils upon the second; that feeling in the lips depends upon the first, and the action of the muscles of the lips upon the second; and, finally, that the sensation of these muscles, which the second sets into action, depends upon the first."

"We observe that the portio dura rises near the portio mollis; and we recollect that the motions of the ear depend upon the former, and the sense of hearing upon the latter."

"We observe again, that the sixth nerve rises

near the fifth; and we recollect, that it directs the eye outwards towards the orifice of the lachrymal gland, the secretion of which is under the control of the fifth."

"It is unnecessary to point out, in detail, how completely the glosso-pharyngeal and the pneumogastric nerves support the principle which I have endeavoured to establish. That every nerve cannot be arranged under it (for I knów not by what means it can fairly be made to include the first and ninth) does not, I hope, render the observation valueless."

I trust, that with the comments which I have thus made, the views which I have concisely stated in my other writings as to the elementary functions of the nervous system, and especially of those of the segments of origin, or ROOTS OF THE NERVES, taken either separately, or in isolated groups, or in subservience to the medulla oblongata, will be now intelligible enough, and appear to harmonise with the theory of the remaining parts of the nervous system, the whole of which I have collected in the fourth edition of my "Outlines of Physiology."

I shall now proceed briefly to enumerate the powers of the roots of the nerves *in disease*, or the corollaries explanatory of symptoms, which may be deduced from the fact proved by my experiments added to those of older physiologists, that *each segment of the double cord, with the nerves arising*

from it, contains the entire mechanism of sensation, instinct, and volition, although these endowments in the otherwise perfect state of the segment are not manifested, unless it be in continuity with the medulla oblongata.

The instances, which I have to enumerate, are of two kinds,—in one, the segments of origin are raised to a præternatural excitability; in the other, their force and excitability are lowered.

I. To the first class evidently belongs chorea, or St. Vitus's dance. This malady is often met with, in conjunction with a state of mind perfectly steady and composed. It is the mechanism of motion, which alone is disturbed. We may conclude the segments of the double cord, which originate nerves, in it to be præternaturally excitable. The patient wishes, perhaps, to nod his head, and a motival stimulus is deliberately sent down from the brain to that effect; but it falls upon highly excitable segments of origin; and off at once are sent, instead of the voluntary impulse to a single grave bend, fifty rapidly succeeding impulses to the muscles to nod the head in every direction but the right.

Tetanus, again, as I originally concluded in 1823 (see extract, page 20,) and as Dr. M. Hall has subsequently proved by an original experiment, consists in a sustained præternatural excitation of these segments.

In hydrophobia, the pathognomonic symptom of the disease flows from the same source, irritation of one segment of the double cord,—that, namely, from which the glosso-pharyngeal and pneumogastric nerves arise.

In some forms of hysteria, the influence of the same principle is equally obvious.

The convulsive attacks of infants,—spasms of the hands and feet, spasm of the glottis characterised by the crowing inspiration which attends its departure, spasmodic tension of the back and neck,—are equally palpable results of an iritated state of the segments of origin of the nerves situated in the medulla oblongata and spinal marrow.

II. The occasions in which the powers of the roots of the nerves, or of the segments in which the nerves rise, are overborne, are not so various; I shall content myself with pointing out two.

Sometimes the arms are palsied, or permanently contracted, without the lower part of the body being affected. In this case, the segments which originate the nerves of the arms have been found, upon dissection, wasted, while cords of white matter have passed over them to the lower part of the spinal cord, the segments in which had not wasted, and had preserved their powers through the continuity thus kept up with the medulla oblongata.

But nothing is so common as hemiplegia, or palsy of part, or of the whole, of one side of the body from cerebral disease. I think I have succeeded in showing how all its features may be deduced as consequences, from principles already explained, assisted by the application of another anatomical fact, which, however disputed by some, and multiplied by others, is as Santorini first stated it.

The segments of origin *habitually excited to action by impulses from the brain*,—receiving from the deliberative judgment, imagination, appetite, therein residing, the motival impulse to the will,—are liable, as I conceive, to have *their energy utterly subdued* by influences proceeding from the same source. The brain, in certain states of disease or lesion, is capable of despatching a withering influence, which strikes all power temporarily or permanently out of the segments of the cord.

Now, the channel of that palsy stroke is very evident, for the palsy affects the opposite side of the body to the side of the brain diseased. In other words, it moves along some channel, which passes over from one side of the brain to the other side of the double chord, to affect the opposite series of ROOTS of nerves. But the only channel which so crosses over is the anterior pyramid, and along it we must suppose the *palsy shock* to be conveyed. Besides this theoretical necessity, there

are facts, which I have collected in my "Outlines of Pathology," to prove that this *is* the channel of transit for the palsy stroke.

The usually received opinion upon this subject is, that when a cerebral lesion produces palsy, it acts by interrupting the customary supply of sensorial or cerebral power to the spinal cord. But this opinion cannot be maintained; for if it were true, it would follow that the removal of one hemisphere of the brain, or its lateral compression in an animal, (from which a large part of the cranium had been removed,) would produce hemiplegia: and an acephalous infant, or an animal with the cerebrum and cerebellum removed, should be completely palsied, which is contrary to fact.

Another consequence of this mode of influence, were it the true one, would be, that in palsy strokes the parts the most remote from the brain would have their supply of nervous energy soonest cut off, which, again, is contrary to fact,—inasmuch as the arm is, in almost every case, struck first and more severely than the leg, and when recovery takes place, is slowest in being restored.

But if the cause of the palsy be, as I conclude it to be, an actively depressing influence, propagated from the diseased brain to the nuclei of the segments of the medulla oblongata and spinal cord, the difficulties, which attended the explanation of the symptoms upon the old hypothesis, are done

away with. It is easy, on this supposition, to explain why the arm should be smitten first, and most severely; and why the leg, if smitten at all, should be the first to recover. The fasciculi of one anterior pyramid I have shown to be implicated in the closest manner with the centre of the summit of the spinal cord. A shock, transmitted through the filaments of the pyramid, would, it may be presumed, tell with the greatest violence where it would first fall, namely, on the segments of the spinal cord, which originated the nerves of the neck and arm, and would tell *down* the cord in a proportion lessening with the distance, from the insertion of the decussating fibres of the pyramid in the cord.

But how can such a shock affect the nerves and muscles *of the face*, on the opposite side to the cerebral lesion? I have shown that the fibres of the spinal cord, with which the decussating fibres of the pyramid are implicated at their insertion, in their upward course, ascend directly to the point of origin of the nerves of the face and throat. Is it surprising that the palsying shock, falling at a point so near this, and in continuity with it by reflected extension of nervous substance, should propagate *upwards likewise* its withering influence, and paralyse the seventh, eighth, ninth, and fifth nerves, one or all? But nothing is commoner than these very incidents in hemiplegia, occurring

alone, or in conjunction with palsy of the arm. The lower extremity on the one hand, the orbital apparatus on the other, are the points of extreme remoteness from the centre, at which I have supposed the palsy shock to be delivered. It is remarkable, and forcibly consistent with my hypothesis, that these parts should be proportionately so much seldomer affected, than the face and arm. It has been objected to this view, that in hemiplegia, from cerebral disease, the leg is sometimes affected alone, or in a greater degree than the arm. If this occurrence were frequent, it would invalidate my hypothesis. Occurring, as it does, very rarely, it admits of many explanations; one of which is the following. Suppose the lower segments of the spinal cord to be already in themselves weaker than the upper, and disposed to fall into paraplegia; then a slight hemiplegic shock supervening would naturally produce the result observed; the weaker part would suffer first, though most out of the way of harm.

Such are the heads of the hypothetical solution of the phenomena of palsy, which I have linked with the preceding facts and principles. With it I close this commentary on the "Powers of the Roots of the Nerves," referring the reader, for a fuller consideration of their influence in disease, to my "Outlines of Pathology."

ON THE SO CALLED

MAGNETIC SLEEP.

I became acquainted with the Baron Dupotet, through Dr. Roget, and being curious to see the means by which he professed to produce the marvellous results ascribed to animal magnetism by its votaries, I invited him to visit the Middlesex Hospital, where he made trial of his art on four patients. I have since been present three times at the residence of M. Dupotet, in Orchard-street, and have witnessed the application of his method to several other patients; and I think, that what I have observed of the effects which ensued, is sufficiently curious to interest philosophical readers.

I set aside the consideration of M. Dupotet's pretensions to produce *clair-voyance*, and the like, which with the whole theory of an animal-magnetic fluid, are, of course, purely visionary. The means by which M. Dupotet acts, are the influence, sensibly exerted upon some temperaments, of stillness and silence, of the stillness of several persons around and looking on, and of the contemplation of slow and mysterious movements of the hand, executed in perfect seriousness, and with something like confidence in the minds of both parties, that an extraordinary effect is to follow.

The persons, in whom alone M. Dupotet's method has been seen by me to produce any sensible effects, have been young women, who have evidently been more or less disposed to that form of nervousness, which is called hysteria. When made upon others, the trial has proved an utter failure. In the former, again, genuine and ordinary hysteric paroxysms have sometimes been brought on by the magnetising process. But, besides these, a definite physiological phenomenon, a new condition of the system, I have certainly seen induced by the same means in three different persons. M. Dupotet identified this as the magnetic sleep; it is, however, anything rather than sleep, being a kind of stupor, or trance; and as it seems capable of being produced by the manipulations of "magnetism," with tolerable certainty, in persons of a suitable temperament,—having presented at each repetition, which I witnessed, in the same persons the same features, not however, without trivial variations in degree,—and as it is evidently capable of abuse, and possibly might be turned to use, (although of this I am very sceptical,) it may appear to others, as it did to me, not unworthy of examination.

The means employed are the following. The patient is desired to sit down. The operator then, standing or sitting at a little distance before her, raises his hand more or less horizontally to the level of her forehead, his fingers being pointed to-

wards the patient, and at the distance of from two inches, to four, or six, or more; he then moves his hand, at the same distance from the person, down the chest, or down the arms or legs, sometimes keeping it for a few seconds steadily, or with an undulating motion, pointed towards the head, or to the pit of the stomach, or to the knee. Those looking on are requested not to move or speak unnecessarily, so as to draw off the attention of the patient from the operator.

The effect of this procedure on those who are susceptible of the so-called magnetic sleep are the following.

The patient, after a few minutes, closes her eyelids, and appears drowsy; if she is sitting upright at this period of the experiment, her head is apt to nod, upon which, she starts and opens her eyes, and is awake again. If, on the contrary, her posture is such as would allow her to go to sleep without falling, she might appear to a superficial observer to drop asleep; but this event certainly does not happen: the state, so induced, differs from sleep in the following decisive particulars.

1. In the first place, the pupil is not contracted, as it invariably is in common sleep. Upon lifting the upper eyelid, the eye sometimes indeed rolls away much as in sleep, so that the condition of the pupil is ascertainable with difficulty. On other occasions, the eye appears steadily directed for-

wards. In both cases, however, I satisfied myself that the pupil is dilated to the same degree in which it afterwards continues under the same light, when the patient has been roused out of her trance. The pupil is sensible to light, contracting when the patient's chair is turned towards the window.

2. In the second place, the insensibility is in some instances certainly more profound than in sleep. A person in this state does not appear to feel impressions which would awake a sleeping person; so, pinching the hand, or pricking it, produces no evidence of feeling, no motion of the hand, or other expression of uneasiness.

3. The position of the body does not appear as helplessly relaxed as in sleep; there seems to be more tone on the muscles, and the head and neck appear in part self-supported.

4. The patient cannot be roused from this condition as easily as from sleep. The means, which I saw M. Dupotet employ for this purpose, consisted in pressing the forehead and eyelids, and rubbing them, likewise stroking the arms and legs, blowing upon the forehead, and addressing the patient. It was evident to me, that he had to excite a good deal of sensation, *when the trance was profound*, before the patient could break it.

There were evident differences in the degree, to which each of the three persons, from whom I have drawn these observations, were affected.

One of them told me, that she remembered hearing something that was addressed to her when in this trance, (she had, indeed, answered a question that had been put to her, but not very pertinently,) but that she did not remember feeling, when she was touched.

Another of these young persons told me, that the sensation, which she experienced, was like that of going to sleep, and that she believed she went to sleep. This patient is liable to slight epileptic seizures without an *aura;* in these she sometimes loses her senses; at other times, when she is going off, by a strong exertion she dispels the fit. She told me, that the coming on of the trance, in which I saw her on three different days, was totally unlike the epileptic unconsciousness.

I am quite certain that there was no intentional deceit or collusion in these cases; neither were the persons who were the subject of the experiment in any way frightened or agitated. But it was, nevertheless, evident to me that they encouraged the stupor, into which they fell, and strove, to a certain extent, to get into it, and to maintain it; it was not, however, the less real, for being partly their own doing.

The alliance of this state with hysteria is very close, as the following instances, which occurred in some who could not be put to sleep, evinced. One young person, who was subjected to the Baron's

procedure, went off in a regular hysteric fit of sobbing.

Another, considerably older, but by constitution highly hysterical, after sighing and sobbing, became fixed in a sullen stupor, with her eyes resolutely unclosed, in which state she remained for many minutes, and after being roused, fell back into it.

Another, a lively good-humoured girl, who was disposed to laugh at what was going on, was affected in a different way. Her complaint is an hysterical lameness of the right hip-joint. When the Baron directed his hands against her right leg and hip and her right arm, after a few minutes, they began to tremble and move, and at last to jump and be retracted in sudden spasmodic twitches; these motions ceased on the manipulations ceasing, and recurred on their being resumed. She stated, that the twitchings were attended, and as she supposed caused, by sensations of pricking and shooting, which she felt in the limbs at which the Baron pointed his fingers. Of course, these effects arose from the patient being highly hysterical, and her attention being drawn, (or, as old physiologists would have expressed it, from her animal spirits crowding,) to the region attacked by the magnetizer.

I hope the reader will not think these observations trivial; to myself, their verification appeared extremely interesting. In the first place, they ex-

plain the wonders of Mesmerism, without supposing utter and unprincipled falsehood, or egregious folly, in all the parties who have professed a belief in it. I am sure that the stupor which I have described, (which is nearly allied to some forms of stupor which occasionally present themselves in disease, or arise from mental excitement in nervous persons,) may be really produced in the nervous by the means which a magnetizer uses; and likewise, that, when the stupor so produced is not profound, a person may hear and answer during its continuance;—when, if by good luck, the answer is pertinent, and its truth within the range of probability, it serves to cover a hundred blunders. Again, I have little doubt that, when the stupor is profound, a great deal of pain might be inflicted without rousing the patient. I can even suppose it, from what I witnessed, not impossible, that a surgical operation of little severity might be performed on one in this state, and the patient, on awakening, not remember having felt pain.

In explaining the nature of "magnetic sleep," I have used indifferently the terms "stupor" and "trance." Of the two, however, the latter is much the most appropriate. *The magnetic sleep is a kind of trance, into which persons who are susceptible of it may be thrown, by the influence of the imagination excited through the senses.*

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